Elementary Photo-micrography. By W. Bagshaw. Third edition. Pp. 143. (London: Iliffe and Sons, Ltd., 1915.) Price 2*s.* 6*d.* net.

SOME idea of the scope of this volume may be gathered from the fact that about ninety of its pages, which are not very large, are devoted more especially to photo-micrography, and rather more than thirty to photography—that is, developing and printing. The author takes it for "granted that the reader is already familiar with the use of the microscope," and also presumably that he is an amateur photographer, and seeks to show how the two may be brought together without the need for expensive appliances, and furnish results which, "though not perfect, are good and acceptable for nearly all purposes." He succeeds not only by precept but also by example, giving twenty-nine good reproductions of photo-micrographs taken by the simple means that he describes, using only objectives supplied with students' microscopes. These examples are illustrative of the methods dealt with in the text, and include magnifications from 2 up to 4000 diameters, the use of transmitted light, reflected light, a combination of the two, dark ground illumination, the use of polarised light, oblique illumination, illumination by flashlight, multiple-colour illumination, and a photograph on an autochrome plate. They are of excellent quality, including even a photograph of *Bacillus subtilis,* ×1000. But the *Amphipleura pellucida,* ×4000, shows that such simple methods will not serve for an extreme test, although taken by means of a one-twelfth immersion lens of 1·4 N.A. and an oiled-on condenser. By the way, such an objective and condenser scarcely come within the range of "students'" microscopical apparatus. In giving "pre-war" prices for chemicals, perhaps the author expresses his faith in an early return to peace conditions.

LETTERS TO THE EDITOR.

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The Masses of Heavenly Bodies and the Newtonian Constant.

IN a well-known treatise on physics we find the following statement:—"By the third law of Kepler we are led to the conclusion that the same value of G (the Newtonian constant of gravitation) applies to the sun and all planetary bodies." This conclusion appears to be fallacious, as we see by the following elementary considerations:—

(1) Take the case of Poynting's famous balance experiment for determining G. The attraction of the large mass M on the small mass m at distance d is

$$\text{couple} = G_M . Mma/d^2 = m'gl' \quad . \quad . \quad . \quad (1)$$

where m', l' are the mass of the balancing rider and its displacement necessary to counterpoise the gravitative pull of M on m.

Equation (1) gives G_M, for we know all the other factors. The suffix used here denotes that the "constant" G_M only applies to a mass if its temperature is that of M.

(2) The earth's attraction on mass m is

$$mg = G_E . Em/R^2. \quad . \quad . \quad . \quad . \quad (2)$$

where E, R are earth's mass and radius respectively.

Equation (2) gives us $G_E . E$. The earth's mean temperature may be, say, 4000° C., whereas that of M above is, say, 15° C. We have no experimental knowledge that the Newtonian "constant" is the same at 15° as at 4000°. Hence we cannot write $G_M = G_E$ and obtain from equation (2) the earth's mass. It is thus evident that the values commonly given for earth's mass and mean density are based on the unwarrantable assumption that $G_M = G_E$. Thus it is quite possible (for we have no evidence to the contrary) that $G_E = 2G_M$, in which case the earth's mean density would work out to be 2·76 instead of 5·52, as generally accepted.

(3) When we come to the case of the revolution of the earth and other planets round the sun, we have similar considerations to the above. Let two planets have mean radial distances d_1, d_2 and periodic times t_1, t_2, we obtain in the form of Kepler's third law

$$G_S . S = 4\pi^2(d_1^3/t_1^2) = 4\pi^2(d_2^3/t_2^2) = 4\pi^2 k,$$

where S = sun's mass and G_S the Newtonian constant for the sun's temperature, whence we obtain $G_S . S$; as we know Kepler's constant k. We do not know S alone, for we may not write $G_S = G_E = G_M$.

Thus we see that the masses and densities of all heavenly bodies, including the earth, are based on an assumption for which there is no experimental support, and which (considering the great range of temperature involved) is probably false.

In the case of the sun, the stars, and all the major planets the mean temperature is certainly as high as four figures, and in many cases probably five figures, on the Centigrade scale. It is thus inconceivable that any laboratory experiment will ever be made to determine the values G_S, G_P, or even G_E. But it is not unlikely that sure experimental evidence will be forthcoming as to the value of G, say, up to 500° C.

I have recently concluded a long research on the value of G up to 250° C., and I have found an increase in that "constant" of about 1 in 10^5 per 1° C. The full results I hope to publish shortly.

No doubt it has been for the sake of simplicity that astronomers and physicists have assumed constancy in G, and have thus obtained the accepted values for mass and density. But in reality these values (by analogy with the terminology of radiation) are not the mass and density, but the *effective-mass* and the *effective-density* respectively, and would only be true-mass and true-density if $G_S = G_E = G_M$, etc. If any temperature effect, such as is mentioned above, can be firmly established, then these terms ought to be adopted in the interests of accuracy.

So far, for simplicity, we have considered the temperature effect of gravity on the large mass only and have ignored any effect on the small mass. In equations (1) and (2) we have the small mass m at ordinary temperatures, say 15° C., so that we have not to consider temperature effect in connection with it. But in equation (3) the two planets in question may differ in temperature. Even then the equation is correct as it stands, supposing (*a*) the temperature effect on a mass considered as one member of a gravitative couple is identical with (*b*) its effect on mass considered as so much inertia; for these terms (*a*) and (*b*) occur on the left and right sides of the equation and cut out. But, on the other hand, if (*a*) is not identical with (*b*) the equation would have other factors. But neither in this

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case nor in the other can the equation (3) be made to prove the quoted statement with which this article begins.

Although the above considerations are theoretically of profound importance, they will obviously have no influence whatever on astronomical calculations; for in all such calculations the product of mass into Newtonian constant is used. Suppose in the case of the sun $G_S > G_M$ and let S, S′ be the true sun's mass and its present accepted value, then

$$G_S . S = G_M . S'.$$

In no calculation do we require S alone, but always product $G_S . S$, so that the false values for the sun of G_S and S do not lead to error.

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The Spectrum of X-Rays.

(1) KOSSEL (*Verh. d. D. Phys. Ges.*, 1914), discussing the previous measurements of the frequencies of the corresponding lines of the spectrum K and L characteristic for different elements, as well as of the limits of the respective bands of absorption, established certain numerical relations among them which Wagner (*Ann. der Physik*, 1915) confirmed in a more perfect way and even generalised. In addition, the extremely precise measures of Bragg and Pierce (*Phil. Mag.*, October, 1914) for the lines K_α, K_β, K_γ in the Ag, the Pd, and the Rh give additional confirmation.

From among these relations, attention is specially directed to the one obtained by generalising the second law of Kossel—

$$L_\alpha = K_\beta - K_\alpha, \text{ viz., } (1)\ M_\alpha = L_\gamma - L_\alpha$$

In these equations each term expresses the frequency of the lines. With its help we can foresee the frequencies of the principal lines of the spectrum of the M characteristic if L_γ and L_α are known.

If we admit Bohr's model for the atom as modified by Kossel—that is to say, supposing that in the transit of an electron from one ring to another a quantum is emitted, the frequency of which is determined by Bohr's formula, the radiation M will correspond to the transit of an electron from the fourth to the third ring, and the corresponding frequency,

$$\nu = \nu_0\left(\frac{1}{3^2} - \frac{1}{4^2}\right)\left(N - a\right)^2 = 1.59 \times 10^{14}\left(N - a\right)^2$$

will be emitted.

To demonstrate the exactness of this prediction, I have calculated the values of ν contained in the following table by means of the formula (1), starting from the values of L_α and L_γ given by Moseley, Rutherford, and Andrade. The last row contains the values of ν obtained from the formula—

$$(2)\ \nu = 1.39 \times 10^{14}(N - 21.8)^2$$

deduced by graphic interpolation, where N is the atomic number of the element.

	N	ν cal. by (1)	ν cal. by (2)
Pd	46	81×10^{15}	82×10^{15}
Ce	58	189	182
Eu	63	245	236
Ta	73	364	365
Pt	78	438	440
Au	79	452	455
Radium B ...	82	501	505

As can be seen, the formula (2), of similar type to those found by Moseley for K_α and L_α, shows well enough the change of M_α with N, and besides the constant $1{\cdot}39 \times 10^{14}$ coincides with the theoretical value $1{\cdot}59 \times 10^{14}$, between the limits to be expected on account of the method of calculation. No data are at present available to test this hypothesis. The spectrum M is most likely to be observed in heavy elements and in the radio-active bodies.

(2) In the numerical relations obtained by Kossel and Wagner, there does not appear the groups L_{A_2}, L_β, and L_δ of the spectrum L, nor does there seem any simple way of introducing them in another series of formula involving the frequency of the K radiations. This suggests the possibility of the spectrum being composed of two distinct parts, which might be illustrated by supposing that two rings are situated in distinct planes, from one of which (the one corresponding to L_2) no electron can pass to the ring K. The correctness of this hypothesis could be tested by investigating experimentally the lines of the L spectrum that accompany the spectrum K—a study that does not seem impossible.

It is logical to suppose that this group L_2 should be accompanied by others, M_2, N_2, . . . in the exterior spectrum, and even that in these should appear some new ones—M_3, N_3 . . . N_4 . . . In this way it would be easy to understand how the spectrum gets more and more complicated from the X-ray spectrum to the visible, and even the existence of the well-known series in the light spectrum.

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Studies of the Cotton Plant.

I SHOULD be sorry if any reader of NATURE were to be prejudiced against perusal of my book on applied plant physiology, called "The Development and Properties of Raw Cotton," by thinking that I had attempted to write on the systematic botany of Gossypium. Yet this impression might easily obtain, since your reviewer devotes exactly two-thirds of his review to a few pages of my first chapter, the title of which, "The Development of Pedigree," is alone sufficient to indicate that it was not intended for biologists.

Still, technological treatment is no excuse for inaccuracy, even as enthusiasm for herbaria is no excuse for such a phrase as "peripatetic Mendelian crossbreeding of undetermined stocks." Therefore I would direct your reviewer's attention to the fact that my description of Gossypiæ as a sub-order (instead of a tribe) is taken from an accepted authority on systematy.

He states that "the results and evidences of systematic botany" with regard to cotton have been before us for a long while; the Hindi weed-cotton to which he purposely refers is a sad example of the result. I regret being obliged to mention it, but he misspells it as "Hindu" weed, and then objects strongly to my leaving it with such a loose designation. Now, in the first place, this name is on record in Sir George Watt's standard monograph of the genus, and is therefore as definite as any other. Possibly more definite, for though the specimen used for that determination came from a pure strain, Sir G. Watt called it "a ferine hybrid possessed of a strain of *G. vitifolium*." Only by enlisting the aid of "peripatetic" Mendelism can one justify the apparent contradiction, but nothing *a priori* can excuse the inclusion of this Hindi weed in the "fuzzy-seeded cottons" during the primary division of the genus in that monograph. There is no cotton which has a more naked seed than Hindi.

It is clear that I took too much upon myself in attempting to persuade the growers and spinners of

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